to apply to mulattos, and some doubt is thrown on the case cited by Wells. On the other hand, Lawrence quotes from the Philosophical Transactions ("v., 55") a case of two negroes who had a white child, the paternal grandfather being white. This seems purely Mendelian.

November 25. W. T. THISELTON-DYER.

Specific Stability and Mutation.

THE desire to be as brief as possible has led, I fear, to some obscurity in the sentences quoted by Sir William Thiselton-Dyer (p. 77) from my letter of October 17. The meaning will perhaps be clearer if I explain the precise significance which I attached to the words "appear" and "occurrence."

By the occurrence of a mutation in one of the higher plants I meant the production of a seed capable of germination and containing an embryo with definitely different potentialities from those of its parent. The appearance of a mutation, on the other hand, implies that such a seed has germinated and given rise to a plant recognisably different from other members of the species. My contention is that the conditions of cultivation are such as to allow of the safe germination and growth of plants which would have no chance of survival under natural conditions. It is therefore possible that mutations may *occur* as frequently under natural conditions as under cultivation. This being so, it does not appear to me to be an abuse of language to state that the assumption that cultivation causes the occurrence of mutations is one which requires proof. In support of this assumption Sir William Thiselton-Dyer brings forward certain evidence. With much of this evidence I was already familiar, but it did not appear to me to amount to satisfactory proof of the current position. The authority of eminent breeders is quoted for the fact that, as soon as one new variety of a cultivated species has been obtained, a host of others immediately follow. But the explanation of this may be that the breeder, as soon as he has obtained a single novelty, immediately crosses it—deliberately or by accident—with the original type, thus giving rise to endless new combinations. R. H. LOCK.

Botany School, Cambridge, December 2.

THAT mutations inevitably appear sooner or later under cultural conditions is not an assumption, but a fact. That they do so only casually under natural conditions, and usually fail to perpetuate themselves, equally seems to me not an assumption, but a fact. If, as Mr. Lock seems to argue, there is an equal chance of their *occurrence* in either case, then their *appearance* should be more frequent in nature than in cultivation, because the former has a larger population to work with. But it is not so. I therefore conclude with Darwin that cultivation introduces some provocative condition which is lacking (or latent) in nature. What that condition is seems to me a very important subject for research.

December 5. W. T. THISELTON-DYER.

The Winding of Rivers.

WITH your permission I would like to make a few remarks on the winding of rivers, which is at present being discussed in your pages. My observations were made while fishing, and my remarks refer to the rivers of our own country, and may not apply to rivers of greater volume. But first I would like to point out an objection to Prof. J. Thomson's experiments. In Prof. Thomson's paper in the report of the British Association for 1876 no details of the conditions of the experiment are given, but Sir Oliver Lodge in his letter (NATURE, November 28) says Prof. Thomson's model had a wooden bed. Now it is very evident that we must be careful in drawing conclusions from experiments made under these conditions. That wooden bed, however carefully made, would not be of the shape that nature would have given it, and any deviation from nature's shape would cause unnatural currents. It, however, does seem probable that something of the nature of Prof. Thomson's diagonal under-tow will exist even in river-shaped beds.

The whole question of the flow of water in river beds is extremely complicated. This is evidenced by the contrary results of the observations of your correspondents. But little consideration is sufficient to show that this must be so; the variables are so many. We have, for instance, variations in the curvature of the bend, in the velocity of the water, and in the formation of the bed of the river, which we must remember is dug out and shaped by flood water for flood water, and is but little altered as the river falls in volume. Take, for instance, the case supposed to be represented by Prof. Thomson's model. Here, with a certain curvature and a certain velocity of flow, we can easily imagine the formation of the diagonal under-tow. But if we were to increase the velocity of the flow this cross under-current would decrease and ultimately cease, and when a certain relation of velocity to curvature was arrived at we would get the conditions referred to in Mr. R. D. Oldham's letter in NATURE of November 21, where he says:—"Sand and even pebbles may be thrown up to the surface of the water near the outer bank of the stream, and where the waters have overflowed the banks pebbles may be found lying on the dry ground after the flood has passed away."

In most of the rivers I know which flow in gravel beds, where they are constantly cutting away their banks, the main flow is more sinuous at low level than when in flood. At low level the main stream runs into the pools at the bends on the deep or concave bank, and as the deep sides of the successive pools are on opposite banks of the river, the stream has to cross its bed between the successive pools. While in flood the swiftest flowing part, on the surface at least, is near midstream, but the formation of the bed at the place and the flow above and below may alter this in some cases. After the flood has fallen, the river bed it has shaped has an infinite variety of forms at different places, and the flow of the water at any part must be studied with reference to that particular part, and to the part above which has determined the cross-section and velocity of the water coming to it, and also to the formation of the part below which determines its escape.

There is one very common type of flow which frequently presents itself in varying forms in rivers which alter considerably in volume from time to time. After the flood has fallen the river becomes, so to speak, divided into streams and pools. Over the shallows the water runs rapidly, while in the pools it moves slowly and somewhat irregularly. The streams coming into the pools flow next the concave banks, and come into the pools with some velocity, which is soon lost in the slower movements in the pools. The streams thus lose their kinetic energy, which is converted into potential energy, raising the level of the water at the place where the stream loses its velocity. From this part of the pool, in addition to the stream flowing down the pool, a reverse current is started which flows back on the inner side of the pool, flowing to the upper end of it, where it curves round and flows downwards alongside the main stream. Part of the back current is no doubt due to the inflowing main current causing an induced current, but it seems to be mainly due to the loss of kinetic energy of the stream, causing a rise of the level of the water where its velocity is destroyed.

As to the cutting and wearing away of the banks of rivers, that is mainly the result of eddies formed by the flowing water meeting with obstructions, such as stones, tree roots and stems, inequalities in the banks, &c., or even by water impinging on water. One of the deepest pools in a river I observed was entirely dug out of its gravel bed by eddies produced by the main river meeting a large tributary stream at right angles and mingling their waters in turbulent eddies; and it seems probable that the excavation of the deep pools generally found at the foot of waterfalls have been greatly aided by the eddies formed by the falling water meeting the quieter water of the pools.

The common practice in this country of protecting the banks of rivers by means of little piers or "tooks" to throw the water off them, and into the middle of the bed of the stream, generally results in failure, because the piers cause eddies, and deep pools endangering the banks are frequently dug out by these eddies; and while these piers tend to throw the water to the other side of the channel, yet the sloping bed throws it back and causes it to strike

the bank below the pier, thus in some cases making matters worse. The only place I know of where a knowledge of the bad effects of eddies on river banks has been put in practice is in the river Adda, which drains Lake Como, Italy. There the irregularities of the banks seem to have been smoothed to some extent, and then simply paved with small cobble stones a few inches in diameter. Over this the water flows without eddies, and the banks, so far as one could see, gave little trouble, though one would imagine that if a break in this rather weak surface took place destruction might be rapid.

JOHN AITKEN.

Ardenlea, Falkirk, December 3.

May Gorsedds.

IN my communication to NATURE, May 2 (vol. lxxvi., p. 9), I stated that there was another plan of a Gorsedd among the Iolo MSS. at Llanover. The important difference between it and the plan published in that number is the omission of the solstitial stones. It is a May-November Gorsedd pure and simple, based on the equinox, and for that reason very interesting. Both plans are truer to ancient tradition than the present plan favoured by the bards. The present orientation is exclusively solstitial, against the best traditions in point. In the older plans the May-year is given the preeminence in one, and is the only year given in the other. In both the older plans the circle consists of nineteen stones, leaving open a splayed avenue on the east, the breadth of which corresponds to the sun's course from August to November and from February to May. Though the present plan of a circle of twelve stones at equal distance from each other is antiquarianly sound, one may regard the older plans as still sounder. I have elsewhere shown that the exclusively solstitial arrangement of the stones in the present plan is about the only point in connection with the bardic Gorsedd of doubtful antiquity.

The accompanying tracing (Fig. 1), for which I am indebted to Mr. T. H. Thomas, shows how the original plan was rather carelessly drawn, just the kind of diagram which an old bard would draw to accompany a written description, as in this case, for the benefit probably of an engraver.

In the formal and authentic bardic records very little is said about the significance of the various features of the Gorsedd circle. There is no dabbling in archæology.

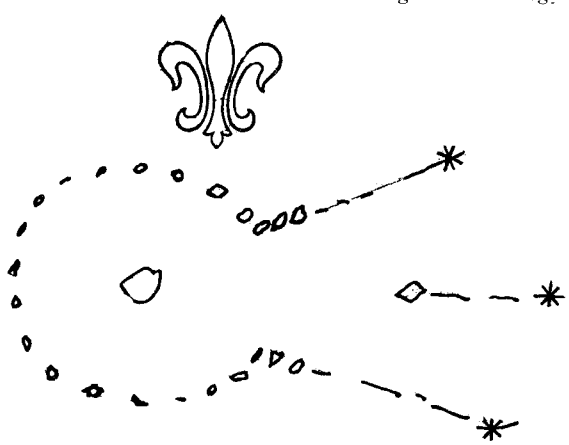

FIG. 1.—Iolo's May Gorsedd.

It is enough for the bard to be able to say that everything he records is sanctioned by immemorial custom.

In the second quarter of the last century a bard arose who claimed also to be a chief bard or archdruid, having the bardic name "Myvyr Morgannwg." He attempted a scientific and philosophical interpretation of the Gorsedd. He insisted upon the absolute identity of the bardic institution with the circles of the Stone age. He made several successful hits at the truth about the Gorsedd, but wild speculations and irrelevant matter have made his various writings hardly readable.

The accompanying diagram (Fig. 2) represents Myvyr's idea of the "most essential elements of the Gorsedd," and is reproduced from one of his controversial pamphlets. It describes a May-November Gorsedd, but with the solstitial signs, except that Virgo is fixed at the equinox. The diagram is true to the best type, but the interpretation is a misfit. It is a forcible illustration of the disturbing effect of a solstitial cult upon sound May-year tradition.

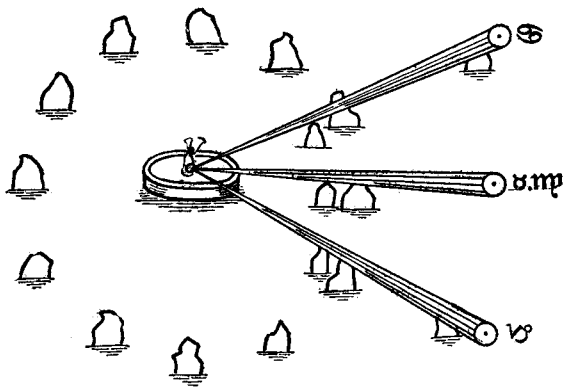

FIG. 2.—Myvyr's May Gorsedd.

Myvyr has also brought his fancy to play on the central stone. A mannikin, holding up something like a bow and arrow, occupies the place of the chief bard, and the three rays look very much like three clubs. Myvyr has nothing to say about the only valuable feature of his Gorsedd, namely, the May-November alignments.

JOHN GRIFFITH.

A FISHING TRIP TO THE GULF OF MEXICO.[1]

MR. AFLALO describes a journey to Florida *viâ* New York, a fortnight's tarpon and other fishing in Florida, and the journey home by way of sundry Central American and West Indian ports. The account given by the author of his outward journey differs in no material respect from numerous extant accounts of similar journeys, but is somewhat marred by a style rather reminiscent of that of the traveller who has perforce to provide his daily or weekly quota of copy for some periodical publication. Such sentences as "In the middle of the ship soft-voiced stewardesses gently raise thick curtains and say that dinner will be up in a minute. It usually is. Fore and aft there is neither curtain nor stewardess, but one sufferer leans across a neighbour of a different race and obeys the irresistible. Everything comes up, even the moon at last . . ." are hardly worthy of a serious volume. There are, however, interesting if slight allusions to and photographs of the Bronx Park Zoological Gardens and New York Aquarium, and a good account of a typical American health and pleasure resort in North Carolina.

Coming as it does from so well-known an authority on sea-fishing as Mr. Aflalo, the second section of the book is naturally by far the most interesting. The account given of tarpon fishing as pursued at Boca Grande is both full and lucid; a sufficiency of detail as to gear, methods of using it, and expenses is given without any needless discursiveness or undue brevity. The whole circumstances of the sport are brought clearly before the reader; the string of boats, each with its armchair fixed athwartships for the angler, towed out to the fishing grounds by a launch in the morning; the fish gaffed long ere they are played to a finish in the fisherman's eagerness to get back to the grounds and kill a larger one; the annoyance resulting

[1] "Sunshine and Sport in Florida and the West Indies." By F. G. Aflalo. Pp. xv + 272. (London: T. Werner Laurie, n.d.).